REBUTTAL LETTER

We thank the reviewers for their comments, which helped clarify our study. We detail below the changes made to address their comments.

Reviewer #1

The article under review covers an interesting topic improving the results of existing literature. While the article is clearly written, in general, some aspects need to be addressed before publication.
General comments:

(1) Reviewer #1

- While the article is generally linearly written and easy to follow, there are a few instances where some concepts are left hanging. A clear example is the shear modulus introduced at line 113 but whose value is defined only at line 194. While the fragmentation of the information may be inevitable due to the number of parameters present in the analysis, referencing the section where the parameters are discussed at length could benefit the readability.

Michel et al.

Thank you for this comment. We now warn the reader at the beginning of Section 2 that the information about the data and their uncertainties are discussed actually in Section 3.

"The data and associated uncertainties used for the constraints are discussed in the following section (i.e. Section 3).''

(2) Reviewer #1

- Some choices introduced as arbitrary could be better motivated (e.g. the linearity of the taper in line 183).

Michel et al.

We changed the sentence to:

"The linearity of the taper implies that the position of the fault's transition to a fully rate-strengthening behavior (>350-450°C) has a uniform probability to fall between 6 km (shallowest position of the 350°C isotherm according to Figure S2) and 18 km depth (deepest position of the 450°C isotherm; Figure S2). "

(3) Reviewer #1

- The results following the analysis using the tapered and truncated seismicity model are presented and compared but they should be discussed more critically, showing which model should be considered for the following studies. Could the results from the two models be combined? How this would affect the results?

Michel et al.

We prefer to not choose between the truncated or tapered model since we do not know which one is more representative of the region's seismicity. In addition, they are equivalent in terms of model complexity and we cannot rely on Occam's razor to chose between models. Nevertheless, we show now in the supplement a figure similar to Figure 5 but with both models combined (Figure R1 and S12). The PDF of $M_{\max}$ has a main peak at 5.9 and a smaller peak at 5.2, which originates from the truncated model. $P(\tau \mid M_w = 5.9)$ peaks instead at ~13 000 yrs.

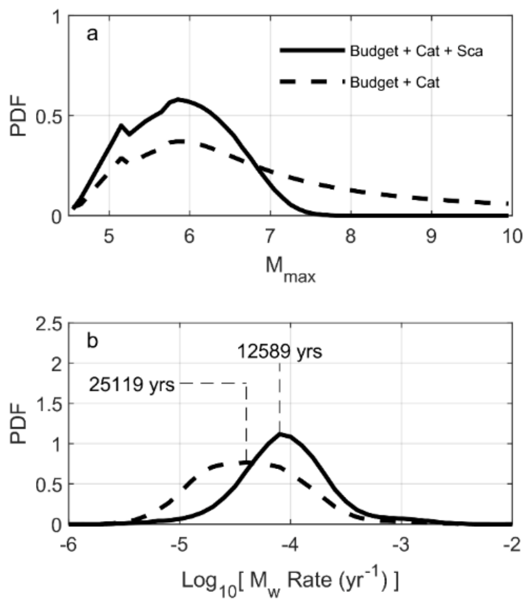

Figure R1: Same as Fig. 5 but using mixture distribution from the tapered and truncated model. (a) Evolution of the marginal PDF of $M_{\max}$ when adding the moment-area scaling law constraint. (b) Same as (a) but for the marginal PDF of the recurrence time of events: $P(\tau \mid M_w = 5.9)$.

(4) Reviewer #1

- While already done for some of the assumptions made, a systematic analysis of the results concerning the different parameters could improve the quality of the paper. How the variation of one (or more) parameter affects the results and, more importantly, how this should shape the uncertainty?

Michel et al.

We now show in the supplement the correlation between M_{max}, the moment deficit rate, the *b*-value and α_s, for both the tapered and truncated model (Figures R2/S10 and R3/S11). We discuss about the correlation between the parameters in Section 4:

"The correlations between M_{max}, the moment deficit rate, the *b*-value, and α_s, for both the tapered and truncated models but without the scaling law constraint, are shown in Figures S10 and S11. For both models, probable M_{max} increases with increasing *b*-value (Figure S10.a and S11.a), highlighting strong interdependency between the two parameters. Raising the moment deficit rate will control the minimum probable M_{max} (Figures S10.b and S11.b) but will also tend to exclude scenarios with a high b-value (>1.25; Figures S10.f and S11.f). While other trends are expected between parameters, they seem less visible, likely due to the uncertainties of the parameters explored. "

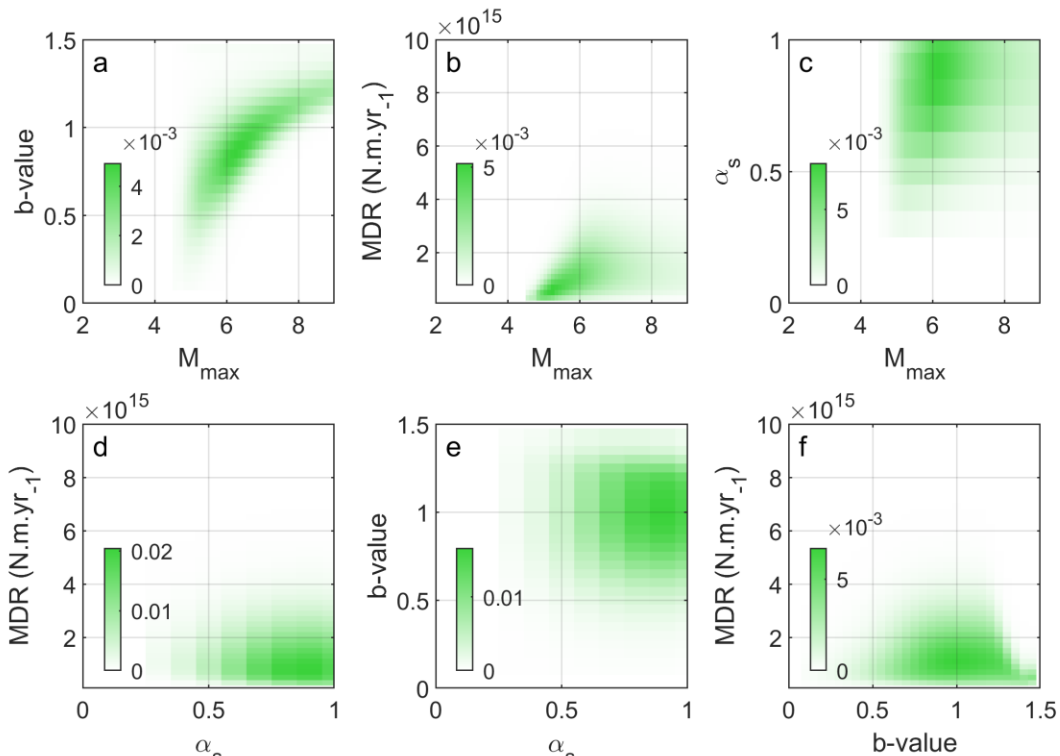

Figure R2: Correlation between correlation between M_{max}, the moment deficit rate (MDR), the *b*-value, and α_s, for the tapered model without the scaling law constraint.

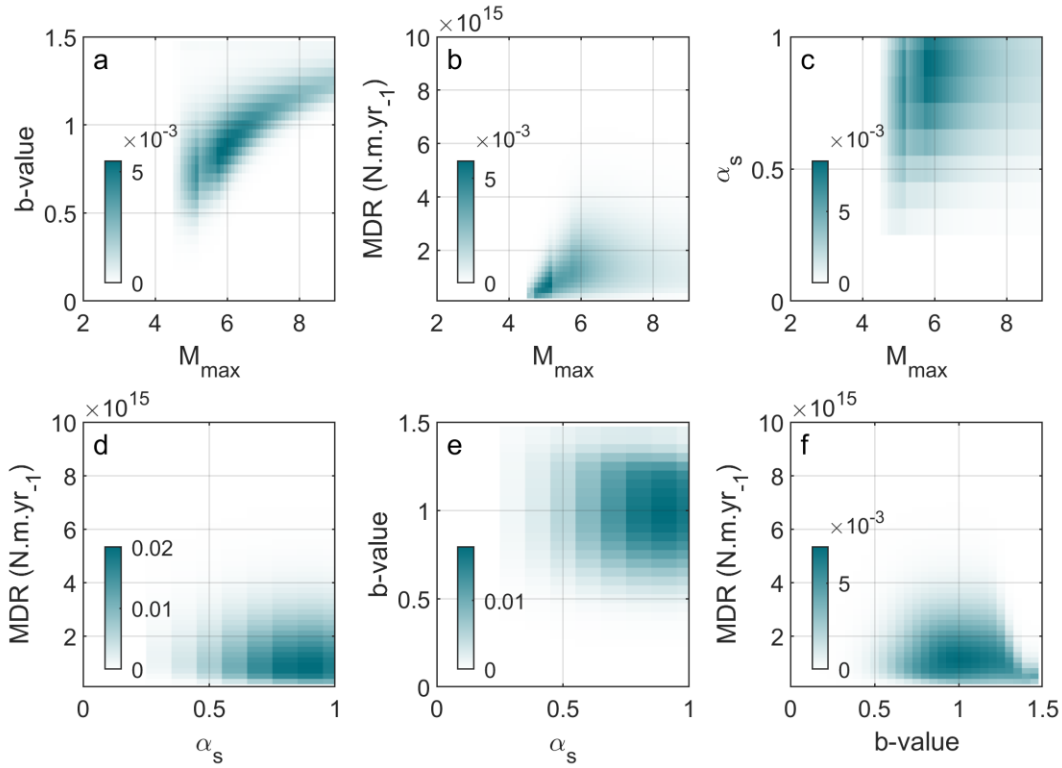

Figure R3: Correlation between correlation between M_{max}, the moment deficit rate (MDR), the *b*-value, and α_s, for the truncated model without the scaling law constraint.

<u>(5) Reviewer #1</u>

- Figures 4, 7, and 8 (as well as S13 and S15) could benefit from the addition of some sort of scale for the PDFs (even though I understand the difficulty due to the figures being already packed with information).

<u>Michel et al.</u>

We apologize to have forgotten to mention it in the caption of those figures. The marginal PDFs on the x- and y-axis are actually normalized by their maximum value and are thus represented with an amplitude of 1. This normalization allows a cleaner presentation on the figure. We keep this format but now refer to this rescaling and give the value of the rescaling for each PDFs in the caption.

(6) Reviewer #1

Targeted comments:

- Is the concept of α_s original or is it taken from previous studies? If the former applies, it should be discussed in more detail; if the latter applies, you should provide some references.

Michel et al.

We now reference Avouac (2015) in Section 2.1 in the manuscript.

(7) Reviewer #1

- In section 2.4 some references are needed relative to how to estimate the parameters from P_SM, as mentioned. In the same section, the variable P_barrier is introduced without being defined.

Michel et al.

We added in Section 2.4 the sentence: "The evaluation of the parameters to estimate P_{SM} are discussed in Section 3". P_barrier is actually a typo and has been removed.

(8) Reviewer #1

- The notation used in line 231 to define the Gaussian distribution N(90%, 25%) can be unclear. Since it refers to a parameter, I suggest changing it to N(0.9, 0.25).

Michel et al.

We changed the notation accordingly.

(9) Reviewer #1

- Section 5.1 provide a clear picture of the variability in the results due to different declustering methods but lacks a concrete discussion motivating how the selection of the algorithm used in the main analysis has been informed. Furthermore, it should be discussed how the results from different declustering algorithms should shape the uncertainty.

Michel et al.

We used in the analysis of reference (Section 4) the declustering methodology from Marsan et al. (2017) because it allows to evaluate the probability of an event to be background seismicity (mentioned in section 3.2). The methodology from Zaliapin and Ben-Zion et al. (2013) does not provide directly such probabilities (it's either background seismicity or not). Although we might be able to modify the Zaliapin & Ben Zion approach to associate a probability to each event, this is clearly not in the scope of this study. In overall, the width of the PDFs using Zaliapin and Ben-Zion (2013) methodology are to first order similar to the results from Marsan et al. (2017).

(10) Reviewer #1

- While the concept in line 374 is clear, the length of future time series (centuries?) needed to improve the results doesn't provide any further information for future studies.

Michel et al.

We removed the sentence:

"Longer time series on all the fields mentioned above might also help in this matter."

(11) Reviewer #1

I also advise the authors to proof-reading the manuscript and the supplementary material: while no major need to be pointed out, there are a few grammatical errors and typos (e.g., the references in the Supplementary material are addressed as "Bibliographie").

Michel et al.

The manuscript has now been read by a native English speaker and we hope it has minimized the number of errors/typos.